In conclusion, I may say that these circumstances do not alter my conviction of fighting on the winning side. The reasons of my faith I hope to give in the next journal of the Royal Scottish Geographical Society. H. B. GUPPY.

17 Woodlane, Falmouth.

### Snow Crystals.

YESTERDAY was very favourable for observing the beautiful appearance of sunlight reflected from snow crystals. As one walked across a field, stars appeared to start forth by thousands from amongst the fresh-fallen snow. They were particularly bright and numerous when one walked in the direction of the sun. They appeared almost at all distances, and almost of all sizes, those near at hand being never very large but of great brilliancy and most exquisite colour. The phenomenon was sufficiently striking to induce me to stop and observe it more closely, and the first thing I noticed on stopping was the permanence of each little star of light, although the snow was dropping melted from the hedges and trees by the heat of the sun. A slight movement of the head was sufficient to change the colour of a red star to green or *vice versâ*. It seemed as if the most brilliant colours were seen when looking in a direction nearly but not quite towards the sun. The level surface of the snow appeared as if strewn with gems—and not only near at hand, for even twenty and thirty yards away a large star would shine forth with a subdued but fine colour. I then noticed a peculiar uniformity of shape in these reflections from snow crystals. The shape never varied from that of a blunt arrow-head. This was very striking in the large stars which appeared at a distance; but once noticed, it was obvious enough that even the minute specks at one's feet were all of this form. Nor did the position of the snow vary to any appreciable extent. The inclination seemed always a little to the right, and this occurred no matter in what direction I looked, whether towards the sun, or away from it, or in any other direction. Wishing to know the absolute size of the larger snow crystals, or combination of crystals, I looked for a fine appearance, and estimated as well as I could its magnitude by covering it with a small object held at arm's length. The distance of the spot where the crystal appeared proved to be forty-three paces from where I stood, and its magnitude could not have been much less than three inches in this particular case. Now if, as I suspect, the form of the star which appears so persistently is due to the upper or lower stem only of a complete arrangement of crystals in an hexagonal shape, these combinations must occasionally be six inches or more in diameter. I did not succeed in recognizing any larger than very minute arrangements of crystals in the snow itself, but it is obvious that the sun's rays reflected from a long distance must single out those faces of crystals which happen to be parallel to one another over a certain limited area. Observation of these reflections, therefore, calls in to our aid a power of analysis in the sun's rays to detect symmetrical arrangements of snow crystals quite unrecognizable by mere inspection. Might I ask for some explanation of the phenomena?

Hull, January 30. A. N. S.

### "The Mammoth and the Flood."

MR. HOWORTH'S letter does little more than travel again over old ground, and two only of the points raised require any notice on my part; the third—the value to be attributed to the opinion of any particular geologist—being immaterial to the main question. As another President of the Geological Society has said: "Science needs no infallible Church, and admits of no Pope."

In regard to the localities in which mammoth remains have been found, I have not "resuscitated" any theory, but have taken my facts from Mr. Howorth's book. His second letter appears to me to ignore a distinction which I was careful to draw in my reply to his former one. That mammoth *bones* should be found at considerable distances from, and elevations above, the existing rivers, offers no difficulty. Indeed, they could not occur, except accidentally, in deltas which are now in course of formation. But, so far as I can ascertain, there is no reason why these "beds of clay and gravel" should not be deposits of rivers which drained the same regions under different climatal conditions, in the distant ages when the mammoth lived in Siberia. The case is precisely similar in England. We should not expect to find mammoth bones in the mud-flats about the mouths of our southern rivers, but in the old valley gravels which occur sometimes even 90 or 100 feet above the present level of the rivers. But the facts most difficult to explain are the occurrences of the *carcasses* of mammoths. It was of these, and of these only, that I was speaking in my letter, as I think would be clear to most readers. No geologist, so far as I know, would deny that the Siberian climate has considerably changed since the mammoth wandered over its tundras, and very likely not seldom got bogged; but the question is, Has it changed suddenly or gradually? The occurrence of the frozen carcass is undoubtedly most simply explained by postulating a sudden change; but when we begin to consider what this means, the remedy, though apparently so simple, seems as heroic as that of the father "who cut off his little boy's head to cure him of squinting." It is then for the best preserved of these frozen carcasses that I suggest the possibility of a drifting and a gradual entombment by the deposits of the ancient rivers. I have again consulted Mr. Howorth's book, and find, between pp. 82 and 89, notices of the discovery of at least ten mammoth carcasses, mostly occurring very far north in Siberia, and nearly all mentioned in connection with rivers: of one it is even said, "like most of the others, it is found on the bank of the river, which had been undermined by floods."

Mr. Howorth further asserts that I cannot have read his book because I charge him with invoking a series of catastrophes when he argues "in favour of one catastrophe only." But, notwithstanding his disclaimer, I would like to know how we are to bring about a deluge to drown the mammoth and a sudden permanent fall in temperature to freeze his carcass (query, one catastrophe, or two?) without "a series of catastrophes." I presume that, as this is a scientific question, we must not invoke a miracle. If continents gambolled like whales—which would be needed for Mr. Howorth's far-reaching flood—would this, unless there were a very special arrangement of continents, so materially alter the climate? and, if they did so disport themselves, what set them dancing? If a number of insular volcanoes exploded with twenty-Krakatão force, this would be a series of catastrophes, but it would probably leave the climate unchanged. If the earth's axis of rotation were suddenly altered materially in position—perhaps the simplest mode of bringing about the two results—would no catastrophic changes be needed to effect this alteration? Mr. Howorth's retort, in fact, indicates better than anything which I can write how completely he has failed to realize the conditions of the problem which he attempts to solve.

But enough. It is impossible for me to continue this correspondence. The reviewer's task is often not a very pleasant one, but a new terror would be added to the work if it involved an interminable controversy with authors on matters of opinion. Dreading this, I deliberately abstained from signing the review, because I knew from past experience that this was my only chance of escape from the flood of Mr. Howorth's controversial eloquence, which, like the proverbial river, *Labitur et labetur in omne volubilis ævum.* YOUR REVIEWER.

### An Incorrect Footnote and its Consequences.

IN following up Baltzer's erroneous reference concerning the "Demonstratio eliminationis Cramerianæ," Mr. Muir, as described in his letter on p. 246, seems at first to have been singularly unlucky. For, on referring to the catalogue of Lord Crawford's mathematical library under "Mollweide," although the work itself was not immediately forthcoming, there was a cross-reference to "Prasse, M. von," under whose name the essay was duly catalogued. The Dun Echt copies, for there are two of them bound up in volumes of mathematical pamphlets, are copies of the original "Demonstratio," in 8 folios, with the pages 4 to 15 numbered, and the last blank. In a gap on the title-page of one copy has been written "auct. Mauricius de Prasse," apparently long ago, and in a German hand. But apart from this the last sentence of the first paragraph identifies the author as the writer of "Usus logarithmorum," which bears the same Latin form of the name in print.

The cross-reference is due to the presence in the library of a little book the title of which is worth giving in full, as it contains the names between which Baltzer's mistake arose, and it also gives the German form of von Prasse's name: it is "Logarithmische Tafeln für die Zahlen, Sinus und Tangenten, neu geordnet von Moritz von Prasse ehemals Prof. der Mathematik in Leipzig, revidirt und vermehrt von Karl Brandan Mollweide ordentl. Prof. der Mathematik in Leipzig. Leipzig,

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1821," ix. + 110 pp. 16mo. In the preface Mollweide says that von Prasse was his predecessor in the Chair of Mathematics, as stated by Prof. Virchl. This work is entered under both the title-names in Poggendorff's "Biographisch-literarisches Handwörterbuch," a circumstance that might have given a clue to the authorship of the "Demonstratio." Strangely enough, this is not the only instance in which von Prasse omitted his name in essays written by him for academical celebrations. I can only surmise that this was done with a view to republication in his "Commentationes Mathematicæ," and that the name was written on the copies distributed as invitations to the celebrations. Whatever the reason, it has in this instance obviously added greatly to the trouble ordinarily experienced when dealing with this class of academical essay, the bibliography of which is so complicated, and at the same time often so important.

RALPH COPELAND.

Lord Crawford's Observatory, Dun Echt, January 25.

## A New Historic Comet?

AT a recent meeting of the Asiatic Society of Japan, a paper was read by Mr. W. G. Aston, H.B.M. Consular Service. This paper will certainly rank high amongst historic papers relating to Japan and Korea. Briefly described, it is a comparison between the ancient records of these two countries and China, and its aim is to establish the relative credibility of these various records. Mr. Aston has so far confined his attention to the period preceding A.D. 500; and his general conclusion is that, as historic writings, the Korean and Chinese chronicles are far superior to the Japanese of the same date.

In the Tongkan, as the ancient records of the Korean kingdoms are called, there is a notice, of which the following is a translation: "Summer, fourth month, Pèkché; comet visible; day-time." The fourth month began on May 14 or 15. At the request of Mr. Aston, I tried to find out if any such comet had been observed elsewhere. The only list of historic comets obtainable in Japan was the list given in Faye's "Astronomy"; and I am not sure if this is meant to be complete. According to Mr. Aston, the Pèkché comet appeared in May or June, A.D. 302. The nearest date in Faye's list is A.D. 295. If this is the same comet, then one *at least* of the dates must be wrong. It is quite possible, however, that both are correct; in which case we shall be indebted to Mr. Aston for having added one more to our list of historic comets. In coming to a conclusion, we must know to what source we owe the knowledge of the 295 comet, and whether this source has greater claims to chronological accuracy than have the Korean records. Not having the references at hand for studying these points, I have written this note to NATURE, in the hope that someone interested in the matter may be able to come to a decision on this question of a possibly new historic comet.

CARGILL G. KNOTT.

Imperial University, Tokio, Japan, December 19, 1887.

## "Is Hail so formed?"

UNDER the above heading in NATURE of January 26 (p. 295) there is a short paper by Cecil Carus-Wilson, in which the writer assumes that under certain conditions, drops of water, whilst falling from the upper branches of a tree, become converted into ice before reaching the ground, whilst other drops falling from the same tree, but at 10 feet less altitude, came to the ground in a fluid state. There is, I think, a simpler solution of this question than the one given. Suppose the following conditions—namely, a frost sufficiently severe as to lower the temperature of the leaves and branches of a tree to a few degrees below the freezing-point; after which a very gradual thaw comes on, accompanied by a fine rain or Scotch mist which freezes on the tree.

Where the leaves and smaller branches hang downwards, small beads of ice would form on their points. As the air became warmer the ice would thaw, and fall to the ground either in the liquid form, or the beads at the ends of the leaves and twigs would become detached in their solid state, and reach the ground as ice-pellets.

Sometimes these ice pellets extend in length, and assume the form of small icicles.

J. RAE.

4 Addison Gardens, January 28.

## MODERN VIEWS OF ELECTRICITY.[1]

### PART III. (*continued*).

### VII.

### *First Representation of the Field due to a Current.*

RETURN now to the consideration of a simple circuit, or, say, a linear conductor, and start a current through it; how are we to picture the rise of the lines of force in the medium? how shall we represent the spread of magnetic induction? First think of the current as exciting the field (instead of the field as exciting the current, which may be the truer plan ultimately).

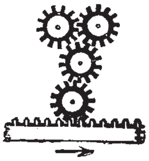

FIG. 34.

If we can think of electricity in the several molecules of the insulating medium connected like so many cog-wheels gearing into one another and also into those of the metal, it is easy to picture a sideways spread of rotation brought about by the current, just as a moving rack will rotate a set of pinions gearing into it and into each other (Fig. 34). But then half the wheels will be rotating one way and half the other way, which is not exactly right.

How is it possible for a set of parallel whirls to be all rotating in the same direction?

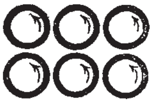

FIG. 35.

If there is any sort of connection between them they will stop each other, because they are moving in opposite directions at their nearest points; and yet, if there is no connection, how can the whirl spread through the field?

Well, return to the old models by which we endeavoured to explain electrostatics, and think whether they will help us if we proceed to superpose upon them a magnetic whirl in addition to the properties they already possess. Looking at Figs. 5, 6, and 13, we remember we were led to picture atoms and electricity like beads threaded on a cord. And these cords had to represent, alternately, positive and negative electricity, which always got displaced in different directions.

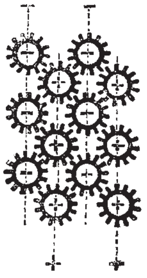

FIG. 36.—Rows of cells alternately positive and negative, geared together; free to turn about fixed axles.

We are forced to a similar sort of notion in respect of the wheels at present under discussion: in order that

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[1] Continued from p. 323.

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